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CANTOR LECTURES

ON

SOME NEW OPTICAL INSTRUMENTS.

BY

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SOME NEW OPTICAL INSTRUMENTS.

By J. Norman Lockyer, F.R.S.

LECTURE I.—Delivered April 28th 1884.

It is in accordance with the oldest traditions of the Society of Arts, that those interested in the use of any scientific instruments should come before the Society from time to time, to give an account of the progress which has been made in them. This does good to more than science, and it interests the Society from more than a scientific point of view. The history of many instruments is as follows.

The first is made roughly by the experimenter himself. Then some artist comes along almost as interested in its success as the inventor. At this stage, the instrument improves rapidly, and possibly some application turns up. Then comes the turn of the industrial arts. From this point of view, my subject is doubly interesting to this Society.

We begin with small spectacle glasses, charmed into a telescope by Galileo. We begin with a little prism, not much bigger than a finger nail, used by Newton, and the work of successive investigators and successive artists has shown the necessity and importance of lenses and prisms of colossal proportions, while the interest in the subjects which can be investigated by means of them is so continually on the increase, that the demand is in excess of the supply.

One would think that it was the time for the industrial arts to step in, but this is exactly what they have not done; and I for one believe that if the Society of Arts, or some such body, does not come to the rescue, the progress of astronomical science will be retarded in this country for many years, as it was before. If I am rightly informed, no optical glass of the size now required (for which almost fabulous prices are given) is being made in England. If I am again rightly informed, the only source of supply in the world now available for us will shortly run dry.

This, then, is my chief text in these lectures, and I shall try to prove my points by some references to history and detail, which I will endeavour to make as short and concise as I can. It is not, however, my only text, for I am aware that the members of the Society of Arts are not interested only in commercial processes.

That one of the improvements in optical arrangements, to which, perhaps, the greatest importance, having regard to the future of astronomy, attaches at the present time, is one the germ of which is to be found in the accounts of the earliest meetings of the Royal Society.

At the time the Royal Society was established, more than two centuries ago, in consequence of the then ignorance of any method for correcting the chromatic aberration of the lens, the aberration was kept down by an inordinate focal length; and lenses with focal length of 300 ft. were attempted to be used. Under such conditions, both observatories and even tubes were of course impossible, and the only movement was the movement of the observer with an eye-piece carried in the hand, and the only possible observation was that of a heavenly body rising or setting.

Hooke, the then Secretary of the Royal Society, pointed out how convenient it would be if, instead of thinking at all of having a tube and then moving it, a plane mirror might be made to move in front of the telescope, which might then be horizontal and fixed. This idea remained dormant until the year 1869, and the reason that it was resuscitated in 1869 was this. Two men, all unconsciously, had been working to the same end: Liebig,* the famous

* Liebig, December, 1867, "Ann. de Chem. et Pharmacie" Sup., vols., 1865-68.

Note.—It may be convenient that I should give here a simple method of silvering glass:—

"Prepare two solutions.

"1. Argentic nitrate is dissolved in distilled water, and ammonia is added to the solution till the precipitate first thrown down is almost entiely redissolved. The solution is filtered and diluted, so that 100 c.c. contain one gramme of argentic nitrate.

"2. Two grammes of argentic nitrate are dissolved in a

German chemist, had shown, in 1867, that it was possible to deposit a clear and bright film of silver on glass ; and the illustrious Frenchman, Foucault, had shown that, by certain optical processes, a glass surface optically plane could be produced.

Here then at last we had the mirror of which Hooke had dreamed. This was not all. The ingenuity of Foucault found a way of mounting this mirror, so that, by means of clockwork, the light given out by any heavenly body could, from its rising till its setting, be poured into a horizontal telescope placed north and south, thus enabling an observer sitting at a fixed eye-piece to do his work quietly, free from any anxiety connected with the movement of either telescope, chair, or dome, so long as the heavenly body was above the horizon.

The extension of this capital invention of Foucault's, and what has come of it, forms one of the chief improvements upon which I have to address you.

The plane mirror in the "siderostat" (such was the name given by Foucault to the new instrument described by him to the Academy of Sciences in 1869) had a diameter of 12 inches. In consequence of the recent researches of M. Loewy and of the Brothers Henry, a mirror of 5 feet in diameter, optically plane, can now be constructed with the most absolute certainty and perfection.

Up to the present time, to satisfy preconceived ideas, it was believed that, to establish rapidly an equilibrium of temperature, it was necessary that the thickness of the mirror should be small. Then, under the influence of a tightening, however slight, or only a flexure, the mirrors were deformed unequally, and consequently produced an obvious diminution in the beauty of the images. In giving to the disc a sufficient thickness, the production of a plane surface is not more difficult than any optical surface whatever. The means of verification are so delicate that, in a mirror of 40 inches diameter, an error of $\frac{1}{50000}$th of a millimetre can easily be determined and eliminated. So if there be sphericity in the mirror, the radius of curvature will have at least 1,600 leagues, that is to say, about the radius of curvature of a bath of mercury; and in taking still greater pains, it will be possible to go further, and to reach an exactitude such that the radius of curvature might be twice as great. Under these conditions it is not possible, as one can readily see, that there can be any appreciable imperfection.

The attention of M. Loewy and the Brothers Henry, as will be seen, has been particularly directed to the thickness of mirrors. For a small mirror the thickness should be $\frac{1}{5}$ or $\frac{1}{6}$ of its diameter, but a larger mirror, say one of 40 inches, need only have a thickness of $\frac{1}{7}$.

With regard to the weight, a disc of 5 feet diameter of the proper thickness would weigh about a ton ; a disc of 40 inches diameter, trimmed down to its proper thickness, weighs 380 kilogrammes.

It is thus seen, then, that for our existing telescopes of 25, 26, and 27 inches aperture, we have already mirrors of which siderostats could be constructed to fill them with light at all usual inclinations; and that, if the definition is not effected detrimentally by the introduction of the mirror, Foucault's plan secures the almost complete realisation of Hooke's idea. There is this to be said, however, that with a siderostat the observation of the whole heavens is not secured. I have now, to direct attention to an instrument that does secure this.

The "equatorial *coudé*," an equatorial instrument in which two of these mirrors are used, to which I wish to draw attention, was designed by M. Loewy, and its construction was commenced in the year 1871, by Mr. Delaunay, who was then the Director of the Paris Observatory, who was much struck with the design. The construction was interrupted by the Franco-German war, and the premature death of the illustrious director of the observatory, and had it not been for the liberality of Mr. Bischoffsheim, it probably never would have been completed.

For modern instruments of existing dimensions—and it is certain that those dimensions will be increased—the construction of a dome is no easy matter. The conditions have to be sought for which will ensure a permanent and easy rotation of a circular roof of at least

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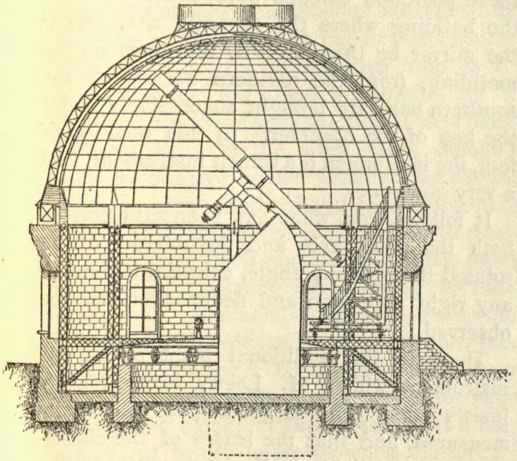
little distilled water and poured into a litre of boiling distilled water. 1·66 gramme of Rochelle salt is added, and the mixture boiled for a short time, till the precipitate contained in it becomes grey; it is then filtered hot.

"The glass having been thoroughly clean with (1) nitric acid, (2) water, (3) caustic potash, (4) water, (5) alcohol, and lastly distilled water, is to be placed in a clean glass or porcelain vessel, the side to be silvered being placed uppermost. Equal quantities of the two solutions are then to be mixed and poured in so as to cover the glass. This should be done while the glass is still wet with distilled water. In about an hour the silvering will be completed. Then pour off the exhausted liquid, carefully remove glass, wash it in clean water, rub off silver deposited where not required, allow to dry. The time required for the operation depends on temperature. If the solutions be warmed to about 30° C. the silver is deposited in a few minutes; but it is safer to use them cold."

60 feet in diameter. Two domes are now being constructed in Paris larger than this, that is, having a diameter of 20 metres. The old system of making the dome travel on wheels and circular rails, or cannon balls and gutters, and the like, has been abolished.

Let me give you an idea, by means of the accompanying drawing, of the two domes to which I have referred. The system is the one suggested by M. Eiffel. The dome is a steel one of twenty metres diameter,

FIG. 1.



and hemispherical. At the bottom of the dome is a circular caisson, also of steel, and connected rigidly with the dome. To the dome, by means of vertical pillars constructed of angle iron, is connected the floor of the observatory. The dome, the caisson, the pillars, and the floor form one whole, entirely metallic, all being firmly knitted together. At the top of the cylindrical wall which forms the body of the observatory is an annular trough holding liquid, in which floats the annular caisson. It is this liquid which supports the whole system of dome and floor, the friction produced by the rotation of these being only that of a ring immersed in liquid.

Experiments have shown that the tangential pull of a few pounds will suffice to rotate the dome, in fact 5 grammes applied to a model, and corresponding to a weight of 5 kilogrammes on the dome, gives a rapid rotation to it.

Below the floor, and running on a rail supported by an interior ring of masonry, there is a system of wheels. The dome and floor are floated, so that the floor is within a few millimetres above this ring, which is only to be used as a resource in case of accident.

The liquid proposed is a solution of chloride of magnesium, which is used by M. Pictet in his freezing machines. It is stated that this liquid does not attack galvanised iron, and is only slightly volatile; with a density of 1·25, it only freezes at a temperature of 35° to 40° Centigrade below zero. The section of the caisson is about 1 square metre, and is made of galvanised iron. The surface of the base is 75 square metres. In taking the height of the liquid line at 85 centimetres, there would be 64 cubic-metres immersed. This will support 80 tons, which is the estimated weight of the dome and floor.

As the floor moves with the dome, and as the observing chair rests on the floor, the observer will be carried round by the movement of the dome, at the same rate as the telescope is carried round by the movement of the clock, and this obviously is a matter of extreme convenience to the observer.

It can easily be imagined that a case like this must cost something. In fact it will cost nearly three times more than the fiddle—I mean the telescope.

The telescope is to cost 183,000 francs; the observatory nearly half-a-million francs; and I am particularly anxious, for reasons which you will see by-and-by, to dwell on this great expense.

Observations made with reflecting telescopes without domes necessitate the astronomer doing his work in very inconvenient attitudes. Lord Rosse's 6-foot, Lassell's 4-foot, Lord Rosse's 3-foot, and the new Paris 4-foot, are cases in point. The position of the observer with Lord Rosse's 3-foot is here shown by means of a model. (Fig. 2, p. 6).

The great point of the equatorial coudé is, that the great expense of domes, and the risk and discomfort of the observer, are at once abolished. The observer can sit down in a comfortable arm-chair, in a warm room, in short, under all the best conditions for securing tranquillity of mind, which is so essential for all accurate observations.

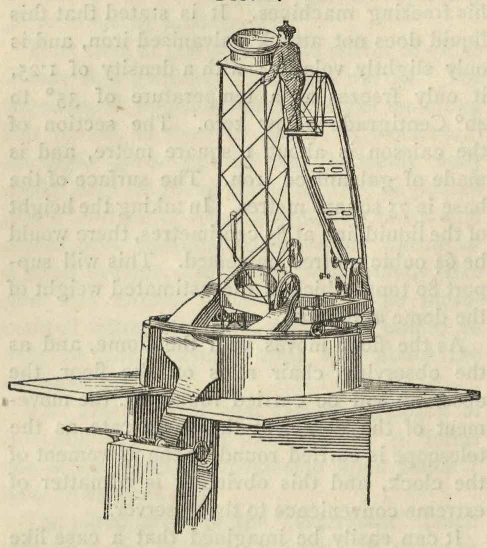
The objects which M. Loewy proposed to himself, in his new instrument, are as follows:—

1. To produce a more stable instrument than the ordinary equatorial, and one enabling large angular measures to be made with certainty.

2. To perfect an arrangement which would enable the astronomer to observe any part of

the sky, and to regulate all the movements of the instrument without quitting the observing chair.

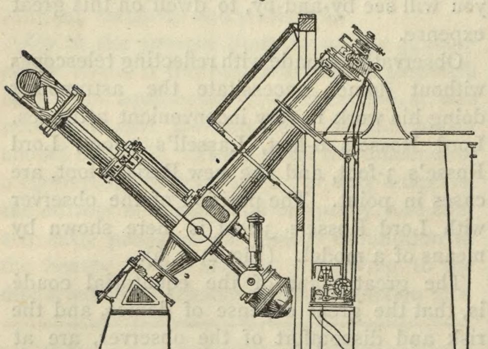
FIG. 2.



3. To avoid the necessity of rotating domes.

We next come to the manner in which M. Loewy has attempted to carry out these conditions (Fig. 3).

FIG. 3.



The polar axis is supported at its two extremities by two pillars. The upper part of this polar axis is hollow.

The principal part of the tube of the telescope is fixed at right angles to the polar axis. With an object-glass at the end of the tube, a right-angled mirror where the tube joins the polar axis, and an eye-piece (Fig. 4) at the upper end of the polar axis. While the instrument turns on its axis, the astronomer sees passing before his eyes the equatorial stars. With the instrument in perfect adjustment, the crosswire in a sweep marks the position of the celestial equator, and this is all that would be seen, but as I said before, the instrument is competent to observe the whole heavens.

How is it accomplished? This is the most important point about the instrument. In front of the object-glass is placed, at an angle 45°, one of the plane mirrors to which I have referred. This can be rotated from the eye-piece.

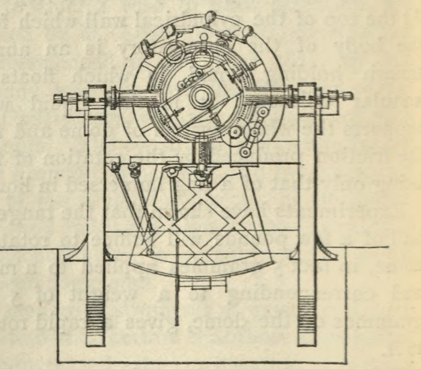
Let us suppose now that the tube with the object-glass is horizontal either to the east or west of the polar axis. The tube in both these positions extends beyond the sides of the building where the observer sits, and if the mirror be then rotated, the stars on the meridian, from the northern horizon to the southern one, are brought successively under the ken of the observer. In this position in fact, the instrument is a transit instrument, and a very good one.

It follows from what has been said that as both the polar axis and the mirror can be rotated through any angle, celestial bodies, in any right ascension and declination, can be observed.

There is this additional point about the instrument which M. Loewy very properly insists upon, that large angles are easily measured, and that the lenses of the object-glass are less liable to displacement. All the external portion of the apparatus, that is to say, all except the upper portion of the Polar axis which carries the eye-piece, is covered by an inexpensive hut which can be rolled away.

The best idea of the convenience of the arrangement, so far as the observer is concerned, will be gathered from the view of the

FIG. 4.



eye-piece. In the centre we see the micrometer with one of the eye-pieces. I shall have a word to say about this micrometer

afterwards, as it is an improvement upon the ordinary form. The position circle and the vernier are seen above the eye-piece. Two other verniers on two other concentric circles are shown above, the outer one being the right ascension one. It will be seen, therefore, that the circles and verniers showing position angles and the right ascension and declination of any object visible in the field of view, are close to the observer's eye. Now, if we wish to sweep round the whole visible heavens in right ascension, all we have to do is to turn a handle. The observer, seated comfortably in his chair, not retarded by the necessity of any simultaneous movement of a dome, sees that part of the heavens which is above the horizon at the turn from east to west, having the same declination as the object at first visible in the field of view defiled before him. On the other hand, should he wish to observe bodies with varying declinations, he turn another handle; this causes the mirror in front of the object-glass to rotate, the telescope itself remaining at rest, except in so far as it is being driven by the clockwork at the time. One handle clamps in declination, and another below the eye-piece, is the clamp in right ascension. To the left of the clamp we have three handles: one handle for winding up the clock, another which gives a hand movement to the telescope when the clock is out of gear, while the third connects the tangent screw driven by the clock with the arc concentric with the circles. The eye-piece—indeed, the upper part of the telescope altogether—is fixed to the floor of the observing room by supports.

Two or three questions now present themselves:—What is the definition of this new telescope with the two additional mirrors? *A priori*, a long focus is an advantage, but the idea of two reflections from plane surfaces will frighten many. All I can say is that MM. Perrotin, Thollon, Trépied, Trouvelot, and Professor Newcomb, have all expressed their wonder at the definition, and that my wonder at it has not been less than theirs.

There has been severe, and in some respects unfair, criticism of the equatorial *coudé*. For instance, Mr. Grubb considers the great objection to lie in the necessity of an outside mirror, and states that no mirror of 40 inch, which would be necessary for a 27-inch object glass, has ever been attempted. This is an error, for such a mirror has been finished. And as he has also misread Loewy's statement of thickness, proposes finally a dialyte, an instrument in which the bi-concave is separated from the bi-convex, mounts this so that all the sky is not included, but retains the coudé arrangement, and uses a mirror with a differential movement to throw the light up the polar axis to an eye-piece in an observing room like M. Loewy's.

M. Loewy, with whom I have discussed this arrangement, objects that with the dialyte arrangement the useful field of view is always small, while it is obvious that astronomical precision is quite impossible with such an arrangement.

My own opinion with regard to the use of plane mirrors is that I should have no hesitation in using an equatorial *coudé* of 20 inches aperture, and mirror 30 inches, and I believe that this will be the form adopted for instruments of precision.

With an object-glass of 30 inches diameter for physical observations, I should certainly prefer the siderostat, thus reducing the cost of an instrument of this size to about one-third of the present price.

With regard to cost, the price of a plane mirror for an equatorial *coudé* is only about a quarter of the price of the object-glass, because although the diameter is much larger, the ratio being as 3 to 2 about, the glass of which the mirror is constructed is not optically pure, and there is only one surface to grind and polish instead of four.

Thus, the price of an object-glass of 27 inches is 70,000 francs, while that of a plane mirror of 38 inches is 18,000 francs. For an object-glass of 40 inches, the price is 200,000 francs; the corresponding mirror of 58 inches only costs 40,000 francs.

LECTURE II.—Delivered May 5th, 1884.

In my last lecture I referred to the use of the plane mirror, in connection with the necessity of movement in those instruments which have for their object chiefly the investigation of the physical constitution of the heavenly bodies. We saw that the use of such mirrors was not confined entirely to this kind of observation, but it was pointed out that wherever there was any necessity for the continued following of a celestial body, then the plane mirror came in, and enabled us to construct a telescope of enormous proportions, which combined not only the maximum of cheapness, but the maximum of comfort to the observer.

I have now to discuss, as briefly as may be, another exceedingly important use of these plane mirrors, and in doing so we must never forget, however large the astronomical refractor may become in time, that the germ of those monsters which are now becoming familiar to us was the one constructed by the lamented Thomas Cooke, of York. It was he who first dared to advance from an aperture of 16 inches to an aperture of 25 inches. How did Cooke construct this object-glass? So soon as he had begun to work at it, he constructed a tower near the walls of the city, where his workshop was, and to this observatory, right away from his workshop, the lens or lenses, one or both of them, were taken when it was necessary to test them, because he imagined that it was absolutely necessary with such large surfaces to test them by observations on stars, and not by using artificial stars, which did very well with smaller lenses; so that, whenever it was a question of determining whether the surfaces were spherical, whether the curves of the crown agreed with the curves of the flat, whenever any aberration was in question, it was necessary to take down this object-glass from the chair on which it was being ground by the machinery, to put it in a cell, to mount it at the end of a tube, and then to go to work with this observing tower. Now it is true that other opticians, among them Clarke, of Cambridge, had shown beforehand that this was not absolutely necessary; that provided the optician had a sort of horizontal well, some 500 or 600 feet long, he could do without stars altogether; but horizontal wells 500 feet long are not common—it is not everyone who can command a dark chamber of that length.

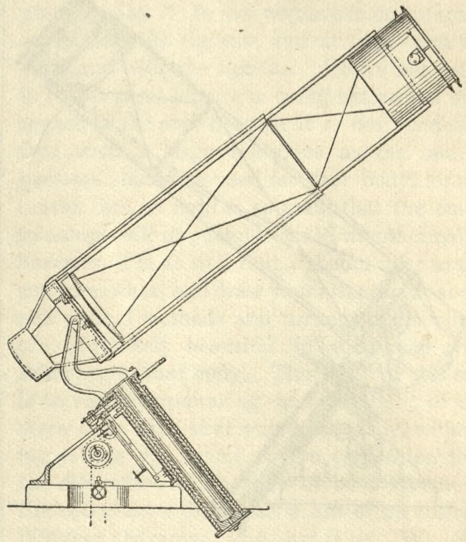
Now, mark what happens the moment you introduce the use of the plane mirror. You mount this plane mirror vertically, put the object-glass vertically in front of it, and, at practically the focus of the lens you wish to test, make a little hole in a piece of wood or of metal, and let light through the hole from a lamp by means of a right-angled prism. Close to this hole mount another lens, by which you examine the image thrown by the lens after the light has travelled first through the little hole, then through the lens, then as a parallel beam from the lens to the plane mirror and back again, until at length it is received by the eye at the eye-piece. With an arrangement of this kind, which does not cost ten shillings, you can test every step of progress in any of your surfaces; and you may test it, whether it refers merely to the perfect sphericity of the surfaces, or to the under-correction or over-correction of the crown by the flint lens which you employ. So that the Brothers Henry, who are now using this system in Paris with object-glasses up to 20 inches in diameter, can deal with the four surfaces—the two surfaces of the flint and the two surfaces of the crown—at the rate of an inch a day. And if you say to them, "I want an object-glass of 16 inches in diameter," they will say, "You can have it in 16 days." Now that time for the construction of an object-glass of that size was not enough when it was a question of dismounting the lenses every time you wanted to test them, and putting them in a trial tube, and waiting for an image of a star during, probably, very cloudy weather.

So that I think the future of this kind of optical work will show that the plane mirror is probably as useful in the construction of lenses as in the use of it to which I drew your attention in the last lecture. I told you, too, in that lecture, that I had just seen in Paris a plane mirror of 40 inches in diameter. This mirror really has been made with such an object in view. It was not made for use with the equatorial *coudé*, although of course it, or one like it, can be easily applied. The point that I had to bring to your attention in the last lecture was that it had been stated that such a mirror was impossible. This mirror is now made as a matter of business, not for any use directly of its own, but for its indirect use in enabling the very rapid testing of object-glasses to be accomplished. And I may say this particular plane mirror of 40 inches to which I have referred is wanted for those two object-glasses of 28 inches and 29 inches diameter to which I have also referred; and still having my text in mind, as the preacher should have, I have now to point out that that mirror is useless, and that those object-glasses are not being finished, because in the wide world the application of the arts has not succeeded in enabling opticians to procure crown glass of 30 inches in diameter.

Having the same ideas in our minds, let us change the *venue* of them slightly by passing on to reflectors. We discussed the mounting of reflectors in the last lecture somewhat, and it was pointed out that all the mountings which we have had, however good or bad they might be, have this terrible disadvantage connected with them, that the observer was hardly ever in the best possible conditions for making observations. I have now to bring to your notice another form of equatorial mounting of reflectors, which I think opens out a perfectly new use for that instrument. I do not mean to say that with this new mounting the observer will be absolutely comfortable, but I do say that it possesses an enormous number of advantages over the old forms, and it is one which, from our particular point of view, is well worth studying. By the kindness of Mr. Common, to whom this new form of mounting reflectors is due, I give two drawings, made to scale by him, which will give an idea of the difference between this form of mounting and the other ones to which I drew your attention.

Fig 1 is the mounting of a telescope of 3 ft. aperture, which is now at work in the neighbourhood of London, and the first point about it is that as Mr. Common had the very happy idea of floating its polar axis in mercury, the movement of its polar axis is perfectly easy, so that this telescope, although we have a distance of 3 ft. from side to side, and a distance of from 18 to 20 ft. from end to end, is moved with the most perfect ease. Another extremely important point about it is that the tube is reduced to a minimum. In the last lecture we saw a model of Lord Rosse's 3 ft. tube, in which the tube was certainly very simple indeed; it was almost a survival, as a naturalist would say, but it was not quite so much a survival as this one. Here we have simply four T pieces beginning with four angles, and the function of the whole thing is to carry the eye-piece, and the weight is reduced as much as possible, while strength and rigidity is retained by means of cross pieces. Why is it so important to reduce the tube in this way? For this reason, that practically the centre of support, and the centre of motion, lie in the mirror itself. That idea gives rise to another matter of very great importance, which is that, as the mirror can lie in the centre of motion, there is no necessity for any great mass of metal between the mirror and the eye-piece, either to abstract heat from the assembly of pieces or to give heat to it, so that one of the chief causes, I think, of the bad definition of reflectors generally

Fig. 1.

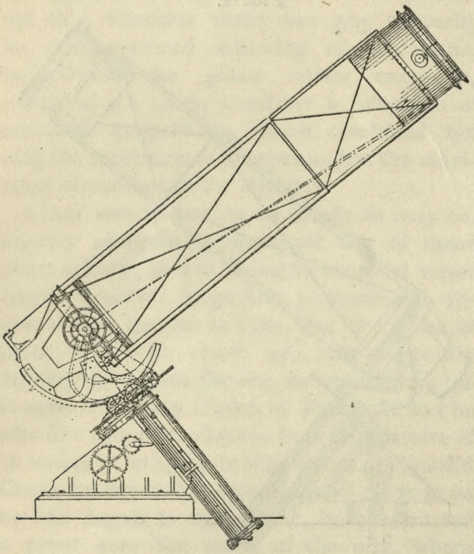


General View of Mr. Common's Method of Mounting, showing Floating Polar Axis.

has been abolished by this arrangement of Mr. Common's. With reference to that point, a comparison of the 4 ft. reflector at Melbourne will best point to the considerable difference in arrangements.

In that, although we have a lattice tube, the centre of motion does not lie anywhere near the mirror, the result of which is that there is a considerable mass of metal between the mirror and the eye-piece, giving rise, in all probability, to convection currents—whether up currents or down currents does not matter at all for my purpose—and giving a very good reason why the disturbance of air between the eye-piece and the mirror should have a very considerable effect. I might have pointed that remark by showing you photographs of

FIG. 2.



SHOWING DETAILS OF THE MOVEMENT.

other reflectors, but I think I have said enough for the present purpose. Now, if you will think the thing out, you will agree to this statement, that with a mounting like Mr. Common's, or something like it, a mirror of 8 or 10 feet aperture is just as easy to mount as a mirror of 3 feet. We should get the maximum of rigidity, and with regard to counterpoising, and ease of motion, what is good so far as floating in mercury for 3 feet is good for the same reason for a mirror of 8 or 10 feet; and here, again, I come back to my text, and say that I believe the only reason in the world that we have not such a mirror at the present moment, is because the industrial arts have not yet succeeded in enabling opticians to obtain a disc of glass 8 feet or 10 feet in diameter to work upon. Now, before one makes a statement of that kind in the theatre of the Society of Arts, one ought to be sure of one's facts. Well, I have done my best to be sure of my facts in regard to that particular, and I will tell you what I have been told by those who are most likely to know. You remember we found, in the last lecture, that to get anything like good and permanent work without loss of definition with a plane mirror, the thickness of it for a diameter over 30 inches ought to be as one in six. Now, it is perfectly true that there is one place in the world—I am sorry to say, not in this country—where they can produce a disc of glass 8 feet in diameter, but that disc of glass can only weigh one ton, and if you come to work it out you will find that a disc of glass 8 feet in diameter, and weighing one ton, will only be 5 inches thick. Now we saw that was a fatal objection. It must be one-sixth; it must be more than one foot thick to be of any use to us as a mirror, whether plane or parabolised. It is thought by some who have studied the question that the matter may be got over for a reflector of the size I have indicated, namely, 8 feet in diameter, by getting two discs run each of one ton weight, grinding two of the surfaces so as to make them plane, then grinding the other surface so as to give it the required curvature, and then to make one mirror by fastening the two plane surfaces together. That is one way of getting over the difficulty, but then, practically, the mirror would be a little too thick, and it would weigh very nearly two tons, so that another method is now being considered, and it is this. Although with our present glass works it is not possible to run more than one ton of glass, it is possible to run very nearly two tons of porcelain, the density of which is very nearly the same as the density of glass; and one question being studied in France at the present moment is this, and it should be a very interesting one to many of you present—whether it is possible, having a disc of porcelain of the required diameter, and of the required thickness, for a large telescope, to put on this disc of porcelain a glass surface which may be subsequently parabolised to turn into a reflecting telescope. I need not tell you that that question is not yet solved, but it certainly is being considered. I should add that with the experience of the French astronomers, and I believe I may say also with that of Mr. Common, who is using mirrors of 3 feet in this country, one really need

not consider the question of silvering the mirrors at all—the thing is perfectly simple. There are two or three ways of doing it. One obvious way which I believe, so far as the information which has reached me goes—I have no personal knowledge of the subject—is quite sufficient, is that the mirror, when you have once got it in a horizontal position, may be surrounded with cardboard, or anything which will hold a certain amount of solution ; then the solution may be poured on to its first surface, as we saw it poured into the beaker in the last lecture, the old liquid being poured off, until at last one has a surface of sufficient tenacity and of sufficient brilliancy to be used for astronomical purposes. So that you see if we only have the glass, it looks very much as if at the present moment we might be utilising an instrument such as the world has never yet seen, that is to say, one of 8 feet aperture, with somewhere about 60 feet focal length.

Well, supposing this be so, should we be any better off than with the existing instruments? Should we be more likely to have these instruments multiplied? Yes, we should, for this reason. In these matters it is almost always a question of money. You have not got the thing because you want the money. The thing is multiplied when it is once shown to be possible, because it is cheap. Now this instrument to which I have referred, if we only had the glass, would, I am credibly informed, be produced in ten months for a sum of £10,000. Now that at first sight seems a great sum; but please remember the facts that I brought before you in the last lecture; and this is one of the reasons why I have brought them before you. The instruments of less than 30 inches aperture which are now being constructed in France—I am sorry to say none of those instruments are being worked here—are to have observatories put over them, which will themselves, without the instruments, cost between twice and three times as much as these instruments of 8 feet diameter would. The estimate for the observatory for this 30-inch telescope is about £35,000. We can get one of these 8 feet telescopes for less than £10,000. I make these statements with some certainty, because, in fact, during my official visit to France, a short time ago, I suggested the construction of such a telescope as this to the French astronomers, and the sums I have named, and the facts I have given you, are based upon actual estimates, and I have no doubt that before very long either in France, or let us say in America—the thing is quite hopeless here — such an instrument should be constructed.

Now, why this instrument is so valuable cannot, perhaps, be stated without reference to some more of these new optical methods to which I am bound by the title of my lecture to allude. The particular new optical methods and arrangements which make this telescope so desirable and feasible, are chiefly two, or rather two sets, the first has to do with the way in which we can now obtain permanent records of the spectra of the stars, and other heavenly bodies. The second has to do with the conditions under which we can obtain permanent records of the appearance of nebulæ, clusters, stars, and the like.

We will begin this, first, by referring to what may be included under the term "spectrum photography." In this we have to do naturally in the day with the sun, and at night with the stars and with the nebulæ. I have not much in this present lecture to bring before you with regard to the sun, because it is not necessary that such a large telescope as the one in question should be used for that body, for the reason that its light is so great that the small telescope will do just as well. Parenthetically, however, it is as well that I should refer to the sun somewhat, and draw your attention to some new optical methods and arrangements which are exquisitely beautiful in their way with reference to that body. The light of the sun is so very overpowering, and the light of the stars is so dim, that you may easily, without my telling you, come to the conclusion that the difficulties which have to be overcome in the two classes of research are of an entirely different character; and that is so. We have to succeed in solar photography by battling with the excess of light—we have to succeed in stellar photography by battling with its defect.

I have here, by the kindness of Dr. Janssen, what in France is called a trap, what we call a slide, and this instrument will give you an idea of the way in which the excess of light in the case of the sun has been dealt with, perhaps in the most satisfactory manner. It is a complicated instrument in appearance, but really it is simple when we look at it. It is intended to deal with the excess of light of the sun by only allowing that excess of light to fall upon the plate for an excessively minute portion of time. This apparatus, therefore, is placed in the telescope between the object-glass which collects the light and the photograph plate, and the only light which can fall on the photograph plate has

to pass through an aperture. Now, in front of this aperture is arranged a sliding slit, the jaws of which can be regulated by means of a screw, so that I may bring the jaws together until they are almost in contact, in case the excess of light is very great, or I may separate them still more by moving the screw in the opposite direction. Then when I have got this slit as fine as I think it necessary to have it for that particular day, I can, by means of a trigger, allow this slit to snap very rapidly over this aperture through which alone the light of sun can fall on the photograph plate. There are several very beautiful contrivances in this instrument, one of them I will show you when I have again set the trigger. Here is the point at which is the opening which allows the light to fall upon the photographic plate, and this is the spring which produces the movement. Now you will see in a moment that if this spring were acting during the whole time that the light is passing over the aperture, the motion of the slit would be constantly accelerated, so that in passing through the solar image one part would be more exposed than the other, and in that way you would never get a proper photograph of the sun, because to get a perfect photograph you want the quantity of light passing through the slit to be always the same. Now that is managed by the length of this spring. The spring ceases to act the moment the slit is in front of the aperture, so that while the slit is passing over the aperture it only travels by means of its acquired velocity; its velocity is not being accelerated. I shall show you presently two or three photographs which Dr. Janssen has been good enough to give me, which show the magnificent results which have been obtained in this way. The most striking result is that he finds that an interval of two seconds is quite enough to change the physical distribution of the bright and dark portions of some parts of the solar surface near the spots. The instrument, in order to demonstrate that, must be so arranged that it will allow two exposures at an interval of two seconds. This is done very beautifully. I am now about to make one exposure; I have now made it, and want to make another; the slit brings with it a shield to prevent the light falling through the aperture until the trigger is again set; the instant that is done the shield flies back, and in that way it is possible to obtain photographic records over a part of the sun's surface, at an interval just long enough to enable you to do this. That then is the way by which the excess of light is got over.

The little patches of light and dark which on the photographs are something like half a second of arc apart, have a distinct existence on the sun, and have a spectrum of their own. That we learned from Sir William Herschel, although of course in his time he was not able to photograph them. But what we have learned from these beautiful photographs of Dr. Janssen, is that they are associated with cyclonic lines, and form part, in a great many cases, of thoroughly well marked and developed cyclonic curves. Dr. Janssen finds, and finds alone by this means—it is not a thing visible to the eye, the eye gets too tired and strained—that these markings, such as you see here, are distributed over the whole sun's surface, and that they vary frequently in exactly the same way as the sun's spots themselves vary. That is one of the results of that method of dealing with an excess of light.

You will understand, of course, that when we have to deal with the stars—and there Dr. Huggins has been as successful in one direction as Dr. Janssen has been in another—we have no longer to deal with an excess of light, but with its defect. And here comes in another beautiful optical method. For photographing spectra of stars, of course we want a spectroscope and a slit in the ordinary way, and what we have to do in order to get the best possible result, is to get it by reducing the part of the plate added on to the smallest possible extent. And while we reduce its exposure to the smallest possible extent, we use as large an instrument as we can, and then see that during the whole time the instrument is pointed to the star, that the light at the focus shall really be entering the slit. Now, it may be a familiar fact to most of you here, that if that exposure has to be, say for an hour, and some of Dr. Huggins's were for hours, no ordinary clock, controlled by electrical influences, will allow the image of any body to remain on slit or on a fine point for that time or anything like it. Dr. Huggins's method, therefore, was to use a Cassegrain reflector in an arrangement by means of which he could, with another telescope, observe the image of the star, and see whether it was on the slit or not; and if it varied however little from the slit, to bring it back at once to its proper line of duty. In that way you see the defect of light was battled with by the time of exposure. The more feeble

your light, the longer the exposure; and that the longer exposure might be effective he wanted a process by which the light should be compelled during the whole time to pass through the slit, and through the system of prisms on to the photographic plate. Dr. Huggins has been good enough to allow me to show you to-night some of his beautiful photographs taken by this means, and I am sure you will be none the less glad to see them when I tell you that that part of the optical method which depended on keeping the image of the star on the slit for an hour together, was played not in all cases by Dr. Huggins, but very frequently by Mrs. Huggins.

This, in sum, is what we have learned of late years in spectrum photography in the case of a defect of light, and you see it is equivalent to what we have learned from Dr. Janssen, in the case of spectrum work, and ordinary work in fact when it is a question of excess of light.

But there are other things to do besides observing the spectra of these different heavenly bodies. We want to observe the heavenly bodies themselves. We want to see what they look like, and to know about them telescopically as well as spectroscopically. Now in this direction I have to refer to Mr. Common's photographs, which have recently been crowned, I am glad to say, by the Royal Astronomical Society by the award of their gold medal. The photographs of nebulæ and clusters which he has recently succeeded in bringing before us are just as valuable in their way, and are fully as important as a method of optical work, as those others to which I have just referred.

When we come to inquire into Mr. Common's work, we find that he is dealing, in some cases, with the very faint markings of nebulæ, and that some of his exposures have been upwards of an hour long; so that we may at once come to the conclusion that in his case also we are dealing with a defect of light. Now how does he get over it? You see there is a very great difference between using a spectroscope as Dr. Huggins did, and a slit; and using simply a photographic plate, as Mr. Common does, on which to receive an image. If we have our spectroscope, and the slit, if the image of the star does not fall fairly on the slit, it does not matter, except that you have to let the exposure go on for a longer time, because the image of the star must fall on the slit for a certain time to get a photograph. But Mr. Common's task was, so to speak, more heroic than that, because in his attempts to get images of nebulæ and clusters of stars, the nebulæ also of course including stars, it is a question absolutely of hit or miss. Dr. Huggins's star, if it were off the slit, did nobody any harm; but if Mr. Common wishes to take a photograph of a nebula with some stars in it, or of a cluster, if he cannot keep each star absolutely rigid on one point of the plate, it is perfectly certain we shall never get a photographic image, either of a nebula or of a star. Mr. Common therefore, having a clock perhaps no better and no worse than Dr. Huggins, was compelled to invent a new optical method or arrangement, and this is it. He has been so kind as to bring it here in order that you may see exactly what it is. In the photographs of spectra which you have seen on the screen, correction was made by altering the movement of the clock slightly, but Mr. Common, after a great deal of trial, found that that would not do; you must let the clock take its course, and you must get some other means of correction. What he did was this. On a plate which had two motions on the eye-piece of the telescope, you have separately first a photographic plate and then an eye-piece. Underneath the eye-piece is a piece of platinum supporting a system of cross wires, and then a system of rectangular screws by which, when you have once got the image of a star on the cross wires supported by the circle of platinum, you can keep that cross wire bisecting the image of a star for as long—in fact, as long as you can. Human effort probably would break down, generally in half an-hour; Mr. Common's human effort, I believe, has already extended to about an hour and a-half. You have then a fine delicate star, bisected by a still finer delicate web; your clock has to be going as well as it may, and your corrections are made by these two screws, with this important consequence, that if you can catch and keep your cross wire on your star in your eye-piece, you have kept the images of the stars absolutely rigid on the photograph plate which is beside the eye-piece.

In this way Mr. Common has obtained a photograph of the nebulæ in Orion. Of course we cannot determine the perfection of the method by the nebula, but we can determine it by the images of these smaller stars. This photograph was exposed for exactly one hour, that is to say, during sixty minutes, or sixty times sixty seconds, the eye-piece had to keep the image of some particular star (necessarily on one side of the photographic plate) by means of the two rectangular co-ordinates worked by the

screws, exactly bisected by the fine cross wire, the cross wire being illuminated by a lamp.

This photograph I consider to be one of the greatest achievements of modern astronomy, and I have taken occasion elsewhere to say what I believe to be perfectly true, that if all the human efforts which have been directed, so to speak, to this group of nebulæ in Orion for two-and-a-half centuries were put in one scale, and this photograph were put in the other, it would weigh them down; in fact, that sixty minutes of nature are worth two-and-a half centuries of art. But that is not all. Not only has Mr. Common, by means of this photograph, shown us that his new method is good for taking a complete picture of that kind, but he has shown us that photography contains within itself, fortunately, the elements of its own correction. What do I mean? I mean that one of the points which now for 2½ centuries—for the work on the nebulæ of Orion began more than 2½ centuries ago—has occupied the minds of observers has been this. Does the nebula change, or does it not? And from their drawings astronomers have not been able really to determine whether there has been any change or not. Professor Holden, in one of the most voluminous and luminous and beautiful memoirs ever devoted to the consideration of one celestial object, a few months ago only, discussed every drawing which is extant, and his conclusion was that really you could not say from any one of those drawings whether the nebula was as it was first observed, or whether it had considerably changed. Now it will be obvious to everybody in a moment that by such photographs as these it will be quite easy to determine in one year, or one century, or a thousand years—art is long—if any change takes place. But we can do very much better than that. Mr. Common has shown that, by employing this method for different exposures, you have a perfect system of correcting the photographic record itself. If you change the salt of silver, which you use of course, you may change at any given time the picture you get. You may be using different kinds of light, and you may imagine a nebula of Orion painted by every wave length, you may get an entire belt of F light, G light, A light, *a* light, H^1 light, and so on; but without going into experiments on variation so far as wave lengths go, Mr. Common's method shows us that by a system which I have called a system of "contouring," to make things quite clear, you will be able in future times to get comparative results, quite independent or almost independent of the salt of silver which you employ. Let us see for instance, how Mr. Common has beautifully built up a nebula. We have here a photograph taken in one minute's exposure, one taken in 2½ minutes, one in 4 minutes, one in 10 minutes, one in 20, and then we have the one of 60 minutes exposure.

With an exposure of one minute we have nothing of the nebula, but we have simply a few of the brightest stars. That is the beautiful trapezium which has been a test object, time out of mind, for observers with small telescopes, and that beautiful line of three stars to the right. Now, passing to the one with 2½ mins. exposure we get them a little brightened: and just a little bit of the nebula round the brightest region. Now the 4 mins. photograph shows you some of the other stars coming out, which were not visible in the first photograph. The next brightens the nebula and also brightens the stars, and we have the nebula in full swing, so to speak. With the 20 mins. exposure it is again increased; and now with the 60 mins. we have the nebula in all its beauty. We have there established an absolute system of contouring, and that can go on quite independently of the telescope, independently of place; independently of everything except the salt of silver you employ.

There is one more photograph I wish to show you. You will see that this method is as good for clusters, as it is for nebulæ. This is not so good a photograph, the method has not worked quite so truly, but it will show us all how important it is for science that it should work well in subsequent cases. The stars should be quite round, and as a matter of fact they are a little elongated, but you see what an immense engine Mr. Common has placed us in possession of, if you take things as you find them; for an astronomer to measure a cluster of stars like that, and give the number of them, and the intensities is a matter which would take years; the photograph I suppose took, perhaps, thirty minutes.

These beautiful photographs have been taken by Mr. Common in a mirror with a 3 feet aperture. Now, if you were dealing with a mirror of 8 feet aperture, of course, we should be dealing with a quantity of light as 64 to 9, and as Mr. Common uses a flat, and as I have shown that a flat is not necessary, we may say practically that with an 8 feet telescope we get ten times more light, and, therefore, the exposure would be one-tenth, so that you see an expo-

sure of sixty minutes would be reduced to six minutes, and, therefore, the labour of keeping the delicate star on the delicate cross wire would be reduced in the same proportion ; further, with such a light grasping power as that to which I have referred, we may hope that some of the stars would give us absolutely instantaneous photographs, and if that were so, we might not only by this means get observations of spectra, but we might get observations of double stars. With regard to observations of double stars, there is just one point which may be argued, and it is this, Mr. Dawes, years ago, showed us that working gradually upwards from apertures of 1 inch to apertures of 30 inches, we enormously increased our separating power on double stars. For instance, he held that with an aperture of 1 in. we could only separate a double star with a distance of $4\frac{1}{4}''$; at 10 in. we could separate stars $\frac{1}{2}''$ apart; and then he calculated that for 20 in. you could separate two stars $\frac{1}{4}''$ apart; 26 in., $\cdot 175''$; 27 in., $\cdot 169'$; and 30 in., $\cdot 152'$; but what would that $\cdot 152'$ become with an aperture of 96 in. We should get down to an exceedingly small fraction of a second indeed, and with a perfect mirror, and with Mr. Common's perfect system, I believe that the time will come when we shall get double star observations added, as a matter of course, to the observations which I have ventured to bring before you.

So that it comes to this, first with regard to the telescopic observations of the heavenly bodies, if we take the nebula of Orion as an example of them, we find that we have in this method certainly a better method than any eye method which the last two and a-half centuries has produced ; that a system of contouring which we can apply in a few hours, will replace the work of months, and give us a record on which we can absolutely rely ; while the work of months, and of years, and of centuries, has turned out to be absolutely unreliable, because it is human. In doing this, then, we establish nebula photometry, but we deal with stars in the field of the nebulæ. We must, therefore, deal with the photometry of stars in the field of the nebulæ, and if we can do that why should we be limited to that field ; why should we not take the average over the whole visible heavens, and bring star after star on the same plate, in the same way as has been suggested by Dr. Huggins and myself in other fields of work. It is absolutely unnecessary, so far as stellar photometry is concerned, that we should limit ourselves to any one nebula, or to any one hemisphere, and it is perfectly simple to get one photographic plate, and to put on it a dozen or twenty images of different stars which will give you, in terms of a certain salt of silver at one epoch, the different radiant energy of those stars for comparison in future time. Now, it is perfectly certain that if this is so, work like that is far superior to any work which any observer, however eminent, might accomplish, because the thing would be absolutely beyond all doubt, whereas one might doubt the observer. If we pass to spectroscopic work, the spectra might be chosen from every part of the sky, and you may have, as you have hypothetically for the intensities of the luminosities of the stars, the spectra of different stars culled, so to speak, from any part of the heavens you choose ; and if you can get a dozen spectra of stars, culled from near or far regions of the heavens, on one photographic plate in a single night, is not that better than any work that one observer can do with that telescope? So that I would propose that, with this 8 ft. telescope, the observer should be absolutely abolished, that you should not allow the telescope's time to be wasted by being employed by an eye which can only see one thing at a time, and which after all may not be quite certain what it sees, or give rise to doubt when other eyes in future years come to examine what it has seen. This 8 ft. reflector I would make an instrument fitted only for such researches as those which Dr. Huggins and Mr. Common have shown us to be possible. I would then, by means of electro-magnets or what not—it is a perfectly simple thing to settle—get an arrangement by which a photographic plate could be sent up, capable of coming down again after a certain time, with ten, twenty, or thirty different images impressed upon it if you wished ; if it came down with one photograph as good as those Mr. Common has enabled me to show you to-night, I, for one, should be perfectly content ; but if you want quantity, it is quite easy to get it by lengthening your plate. Why do I insist upon this ? For this reason, that the observatory is no longer necessary. This enormous instrument, with an 8 ft. aperture and 60 ft. focal length, can be sheltered by a hut costing £100, instead of being rendered useless by an observatory which would cost £40,000 : and I, for one, believe that if we had such an instrument as that, made on such lines as I have indicated to-night—thanks to

the specimens of work which Dr. Huggins and Mr. Common have allowed me to bring before you—one year's work with it would make the year's work with more expensive instruments absolutely ridiculous.

Then, to conclude, I refer to my text, and wish to bring pointedly before the Society of Arts the fact that this dream—this beautiful dream—lacks all chance of realisation at the present time for want of glass—for want of the applications of the arts to astronomical research.

A vote of thanks was passed unanimously to the lecturer.

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